Interactive comment on "Climate and topographic controls on pasture production in a semiarid Mediterranean watershed with scattered tree cover" *by* J. Lozano-Parra et al.

We want to thank Dr Scott for his comments on the manuscript and for his editorial suggestions. In this reply we hope to clarify the reviewer's questions. We also indicate where we will make changes in the revised manuscript based on Dr Scott's suggestions.

D. Scott (Referee)

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I found this to be a thorough and complete study that is well-written & carefully referenced. The paper is easy to read and follow. The modelling exercise appears to be described in sufficient detail to give readers a clear idea of the model structure and function, while it strikes a good compromise between adequate and excessive information.

There are some small errors relating to correct use of English, but these can be readily corrected and do not involve major work. I'm attaching a scanned copy of the manuscript that I annotated to point out these errors and suggested corrections.

I believe that the title is misleading as it does not indicate that the study is a modelling exercise. This point needs to be made clear in the title so readers know that the results are the product of simulation. I'd suggest that the title include the word 'modelling' and perhaps the '300 year series'.

Reply:
We will made explicit in the title that this is a modelling study. The title will be revised to be along the lines of: 'Climate and topographic controls on *simulated* pasture production in a semiarid Mediterranean watershed with scattered tree cover'. The title may be further edited before the submission of the final paper. We think that there is no need to specify in the title the length of the simulation since it is just methodological details.

I have a number of queries regarding the study:

1. A) In the simulation of the weather data, was the necessary co-variance between variables, that were being simulated separately, considered? (p. 15178, ln 26 & onwards). For example, one may expect that dry conditions would also coincide with larger hours of radiation and higher temperatures. Were the simulations of weather such that these weather variables varied in association with each other?
Reply:
Yes, the stochastic weather generator that we have used in our study conditions the generation of minimum and maximum daily temperature and of radiation to the

precipitation time series. The documentation of LARS-WG describes that the daily precipitation time series is generated from a semi-empirical distribution adjusted to observed precipitation data for each month. The precipitation status of each day is used to condition the temperature and radiation time series. Daily minimum and maximum temperatures are simulated from normal distributions calculated for each day with means and standard deviations conditioned to the dry or wet status of the day. The seasonality of the means and the standard deviations of the temperature record are approximated using different truncated Fourier series for wet and dry days. Cross-correlation between minimum and maximum temperature is preset at 0.6. Similarly, daily solar radiation is simulated from separate semi-empirical distributions adjusted from the available data for dry and wet days. We will clarify this in a section 3.4.

The weather variables not simulated directly by LARS-WG are also conditioned to the variability of precipitation and temperature as explained in section 3.4. Daily longwave radiation was tied to the daily average temperature time series and precipitation using a deterministic relationship (Swinbank, 1964) and therefore dependent on temperature and precipitation. Similarly, relative humidity was calculated from a multiple linear regression using air temperature and precipitation as predictors. Daily wind was found to be uncorrelated to any of the other weather variables and therefore was simulated independently by cycling a measured series of 51 years.

2. A) Where did the tree density come from?

Reply:
Tree density was obtained by manually digitizing each individual tree with a point in a high-resolution aerial photography, then calculating the density of points using a 3x3 moving average kernel. The fraction of the area covered by canopy was calculated using a maximum likelihood supervised classification technique from the red, green and blue components of a 24-bit color submetric resolution aerial photography. The classification success was very high because green canopies where highly contrasting with the dry grass, yellow background. Once a canopy mask was produced, the canopy coverage was obtained by calculating the fraction of pixel classified in each of the larger pixels used in the simulation. A note clarifying this will be included in the methods section of the revised manuscript.

B) Did the tree density change at all over time (through the 300-year period of simulation)? If tree cover was static, and unable to respond to variations in climate, then I think this should be made explicit, as it seems rather unnatural (although not unacceptable in a modelling exercise).

Reply:
We assume that the number of trees is invariant but it is important to emphasize that the 300 year simulation is not meant to be a simulation of 300 years of climate. Rather, the study should be interpreted as a Monte Carlo simulation of the possible range of weather conditions that the site (in its current conditions) is likely to experience in a 300 year return period. Furthermore, in this type of land use (dehesa) the tree cover is influenced by man, i.e. reducing the number of trees transforming the original oak forest to grasslands with a disperse tree cover.

3. The modelling results depend in part on the re-distribution of water both overland (steeper slopes) and subsurface, allowing higher some units to accumulate more water, and thus have greater productivity. However, it was not clear to me how such re-distribution occurred in the modelling. Therefore, it was not clear whether such modelling results should be given much credence. In general, my previous point relates to the need in a modelling exercise to be clear about which results are considered realistic (likely) as opposed to those that might be an artefact of the model design and structure. Running the model over a longer sequence of years will not remove defects or artefacts of the model, but it is the modellers who are most likely to be aware of the limitations of the model. An objective assessment of the model would be a good and useful supplement to the paper.

Reply:

The model takes into account the vertical and lateral redistribution of water and takes into account the effect of topography. Water in the subsurface moves downslope driven by gravity but disregarding pressure effects (kinematic approximation). Water can infiltrate into the soil or become runoff. Runoff can reach the channel or re-infiltrate downslope. Infiltration and lateral subsurface flows are controlled by soil hydraulic properties (hydraulic conductivity, absorptivity, porosity) and by the topographic gradient. When the soil is full, return flow happens. All these processes are explicitly described in the model and lend realism to the results. Still, as in any modelling exercise, some assumptions are made. For instance, the overland flow component assumes that depression storage is negligible and that overland flow is routed through the entire watershed within one day. Other important assumptions are that the bottom boundary of the soil (bedrock) is impervious. These assumptions are appropriate for the study site but will be clearly made explicit in the revised manuscript.

Specific minor points.

- Abstract, ln 9: physical not “physic-based”
- p. 15169, ln 5: derives (or some synonym) and not “incents”
- p. 15170, ln 14: I believe it is wrong to call a modelling exercise an experiment. Shorten the sentence to read “few studies of simulations over the entire range . . .”
- p. 15172. There is awkward language in several places in the descriptions (see attached annotated manuscript).
- p. 15173, ln 10: “crops out” is not correct English
- p. 15174, ln 25: weighed not “weighted” (weighting is to assign a weight or importance to a factor) I did not check the detail of the model description on pages 15175 – 15177.
- p. 15180, ln 1: mean annual precipitation, not “annual mean ppt”. ln 15: represents (?) rather than “present”
- p. 15182, ln 10: change “along the whole year” to throughout the year
- p.15187, ln 19: change “competence” to competition
- In many places the word “production” is used where productivity might be more correct.
- However, on p. 15189, ln 7, the correct word can only be productivity or the sentence is incorrect.
- p. 15190, ln 24: I suggest substituting topographic controls for the longer and awkward, “topographic structure of the landscape”

- p. 15191, ln 8,9: I suggest you end the sentence with the word " . . nutrients." The remainder of your sentence introduces speculation that is not a valid conclusion from your paper.
- p. 15192, ln 18: insert the Chow reference (from next page where it is out of sequence)
- Consider omitting Figures 10 c & d as I don't think they add any value.

Reply:

These figures provide information on the range of productivity in different regions of the basin and this is discussed in the text. Although we would prefer to keep these figures it would be possible to omit them and shorten the text if the editor recommends to reduce the length of the next revision of the manuscript.

- The figure captions, generally, could use some work to clarify what exactly is being illustrated.

We thank again the reviewer for the editorial corrections on the text. These will be incorporated in the text in the revised version of the manuscript.